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Atmospheric Measurement Techniques Discussions

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Interactive Comment

Interactive comment on "Simulation of SEVIRI infrared channels: a case study from the Eyjafjallajökull April/May 2010 eruption" *by* A. Kylling et al.

Anonymous Referee #2

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Kylling et al. present a study of the possibilities to accurately simulate SEVIRI radiance under influence of volcanic ash plumes. The paper is well written and easy to follow. I have mainly two concerns which should be adressed by the authors before the paper is ready for publication in AMT. These are:

a) I find it somewhat strange that all cloud effective radii are assumed to be constant. While the impact of that assumption is not specifically large for water clouds, then uncertainty introduced in the brightness temperature difference is much larger for ice clouds. There are a wide range of effective radius retrievals for ice clouds also form SE-

VIRI, which would make a much better initial guess for the forward radiance modelling than assuming constant crystal effective radius of 40μm. Maybe even the ECMWF fields can add information on this topic if the authors think they do not want to rely on external effective radius retrievals (while they do so for ash through the ash mass inversion based emission fluxes fed into the FLEXPART simulations).

b) I am a bit surprised by the decision of the authors to keep water vapour profiles constant in their assessment over such a large domain (p. 7789 ll. 5f) and to use a rather simple empirical "correction" instead, which moreover has not been developed for SEVIRI channels. I strongly doubt that the assumption of constant WV profiles and constant BTD cutoffs is well justified for the domain stretching so far south. Resulting from this assumption the authors discuss some effects of ash detection possibilities (BTD cutoffs, masking, effect of particle sizes) which in my opinion are minor to the effect of water vapour profiles on BTD. The authors at least have to explain why they think the constant WV assumption is justified and what the caveats of this assumptions are. Given they remain with this assumption (can possibly be done), they should assign it the correct priority in the discussion and not focus on minor effects before even refering to this major uncertainty.

Minor comments:

It would be worth to split section 5 into 5.1 (discussion of ash patterns in radiance field) and 5.2 (discussion of MYSTIC sv. IPA).

p. 7784 l. 20: I wonder what the authors mean with "shadow effects" in an IR study. Moreover in the relevant section they do not use the word shadow at all, so maybe also here it should be reworded to e.g. "parallax effects" or something similar (something what is discussed in section 5).

p. 7785: The method of initializing FLEXPART forward simulations strongly relies on the correct retrieval of SEVIRI ash mass as a priori for getting the emission flux right. How good is the prior SEVIRI ash retrieval in reality? Has there been any evaluation

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of mass column so far? If the authors rely on the ash being a function of retrieved effective radius, why do they not use this effective radius as input to their radiative transfer model? Are the initial SEVIRI retrievals based on the same optical constants (andesite from Pollack et al.) as in this study? These points, although the authors choice may be well justified, need some clarification in the manuscript.

p. 7786 ll. 7f.: As most of the ash in this case study is observed over ocean, I wonder why the authors constrain themselves to the 10.8μm and 12.0μm channels and do not use the 8.7μm channel as well.

p. 7786 l. 24: What exactly is meant with "sulphate particles" here? CaSo4, H2SO4, something totally different? It would be good to specify more clearly which kind of sulphate the authors refer to.

p. 7787 l. 2: It would be good to shortly recalling the main outcomes of the evaluation results.

p. 7787 l. 20: How well does the spherical model represent Eyjafjalla ash particles? Especially in the case of small particles the Mie assumption may be significantly wrong. What is the rationale behind using andesite optical constants (although being of mainly andesitic composition, Eyjafjalla ash also had significant contributions of mafic minerals like pyroxenes and olivines, e.g. Gislason, 2010, Proc. Nat. Acad. Sci.).

p. 7788 l. 1: What exactly is a "voxel"?

p. 7788: As already stated above in the above concerns, I really doubt that assuming constant effective radius for meteorological clouds is well justified, especially in the case of ice clouds. Moreover, the given references of Yang et al. (2002) and Key et al. (2002) cover only shortwave radiation with maximum wavelength of 4.8μm. I wonder how these data are helpful for IR radiance modelling. Yang et al. (2005) describe ice optical properties in the infrared - these data may be better suited for modelling of IR radiance for ice clouds then the shortwave parameterizations.

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p. 7789 ll.5f.: As stated above the assumption of constant water vapour profiles, if used in this study, has to be very well justified in terms of sensitivities and uncertainties arising from this assumptions. I would suggest that it might be better justified to use ECMWF water vapour profiles as they are closely related to the cloud fields used in this study and definitely introduce less uncertainty than assuming fixed profiles.

p. 7790 l. 6: This comment is closely linked to that above: the validity of the assumption of ash being indicated by BTD<0K may strongly vary with the water vapour profile, which has a large influence on BTD, especially in cases with rather weak ash signal.

p. 7790 l. 12: ash mineral composition may also be a relevant factor and at least should be named here, as the spectral behaviour of IR extinction, and thus directly BTD is different for e.g. different feldspars or other silicates (e.g. mafic minerals like pyroxenes are characterized by totally different extinction peaks compared to e.g. quartz / rhyolithic obsidian or highly feldspathic andesite).

p. 7792, l.6: related to the comment above and to Fig. 6, ash containing pixels are characterized by reduced (not necessarily negative) BTD.

p. 7792 ll.13ff.: I assume in this paragraph the authors refer to Fig. 6?

p. 7792 l. 24: To what ash concentration or optical depth does the cutoff value of -0.8K correspond? Is this value suitable to detect also thin ash layers?

p. 7793 ll. 27ff.: Before speculating about wrong residence times in FLEXPART I would suggest to firstly adress the uncertainty introduced by assuming constant water vapour profiles. In reality in this region the water vapour content is likely to be much higher and consequently mask the ash signal in the BTD. It would be worth to comment a bit on that.

p. 7794 ll. 8ff.: as above, nevertheless the conclusions presented in this paragraph may still be correct.

p. 7794 l.22: This statement is not absolutely correct, as theoretically also high mass

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loadings can be achieved by an enormous number of rather small particles.

p. 7795 l. 26: It would be worth to mention here that from bispectral satellite measurements it is rather unlikely to get reliable size information unless the ash composition (optical properties) is known with very low uncertainty. This is usually not the case.

p. 7796: Here I cannot once find the word "shadow" effects, which is referred to in the abstract and in the conclusions. I fully agree with the findings presented here and would suggest to remove the word "shadow" from both abstract and conclusions.

p. 7797 l. 2: What exactly is meant here by "complete"? The model framework is based on a set of assumptions (water vapour profiles, ash composition) and a priori estimates (FLEXPART emission flux inversion based on SEVIRI retrievals, reliability of weather model), which all bring their own uncertainties and may in the future be replaced by (even) more accurate estimates or methods. This would increase the "completeness" of the model framework even more.

Fig. 1: The scaling of the colorbar is totally inappropriate for this figure. If the high extent of column density near the volcanic vent is wished to remain enhanced besides the spreading of the ash over the domain, a logarithmic colour scale would be the better choice.

Fig. 6 (left): I see a lot of black dots but not a single red one.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 7783, 2012.